

SATURDAY, JULY 17, 1880.

A REMARKABLE PECULIARITY OF AN ANTHROPOID BRAIN.

BY E. C. SPITZKA, M. D.

Among the features of the cerebral surface, on whose presence or absence the differential characters of the human brain as compared with that of the anthropoid apes, have been established by anatomists, the so-called transition convolutions occupy a prominent place. As is well known, the occipital and parietal lobes of the human brain are connected with each other by means of short gyri, which bridge over those fissures, which if uninterrupted would separate these lobes like a chasm. First described by Gratiolet as *plis de passage*, and known among English writers as annectant or transition gyri; it is the one among them which borders on the great longitudinal fissure that has been most closely studied.

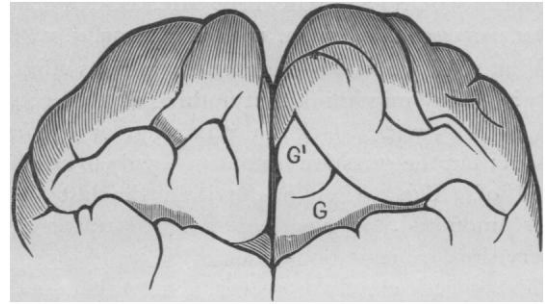
With exceptions to be noted, writers on the human and anthropoid brain agree in stating that the brain of the Chimpanzee differs from the human brain, in failing to exhibit this, the so-called first transition convolution, inasmuch as it is always concealed by the junction of the internal with the external perpendicular occipital fissures.

The same authorities also observe that while there is this sharp demarcation between the brain of the Chimpanzee and that of man, that another anthropoid resembles the human being in this very respect, namely the Orang, in which animal this fissure is present and superficial.

The decease of a large number of anthropoid apes which have been on exhibition at various times during the past few years at the New York Aquarium, and whose bodies were kindly placed at my disposal by the managers of that institution, has enabled me to extend the observations made on the brains of the Chimpanzee and Orang by previous writers.

The first Chimpanzee's brain obtained by myself, differed in no noteworthy respect from those described by Marshall, Gratiolet, Pansch and others, and was utilized for microscopical study.* The second, that of a very large animal, one which had reached the age of puberty, and weighing 389.86 grammes, presented the interesting anomaly I am about to describe, and whose demonstrable existence adds another proof to the many which have been accumulating, that there is no absolute and impassable line of demarcation between the human and simian brain. In the first place, as shown in the subjoined outline diagram, the occipital lobes of this brain are unsymmetrical.

On the right side the internal perpendicular occipital fissure does not coalesce with the external, while on the left side it does. The result is that on the right side, we have an excellently developed first transition gyrus [G, G'] evident and superficial, as in the human being, while on the left side it is concealed as in the ordinary Chimpanzee type. That is, the right side of the brain

Outline diagram of occipital end of cerebrum, dorsal view, $\times \frac{5}{2}$.

shows a higher grade of development than the left. In so far as the left side is usually the better developed one this asymmetry is anomalous. Yet it shows that the old line of demarcation is not a correct one. Though the transition gyrus is concealed on the left side, yet a portion of it is visible, showing that on the whole this brain exhibits a tendency to a more human-like relation.

If we now proceed to compare the transition gyri of an Orang's brain with those of this Chimpanzee's right hemisphere, and of Man, we are struck by the observation that its disposition and proportions are more human-like in the Chimpanzee than in the Orang. And this applies to the Orang in my possession as well as to those figured by Tiedemann, Gratiolet and Bischoff. Taking the occipital lobe of the Orang as a whole, its physiognomy, if I may so term it, is lower and less human like, than that of the Chimpanzee.

The fact that the arrangement of the gyri and fissures bordering on the occipital fissures is thus shown to be inconstant, and that as Vogt has humorously shown, some of the South American monkeys resemble the human being more strongly in this respect than the average Chimpanzee and Gorilla (Pansch), should make us careful in basing fundamental characterization on such slight morphological factors.

I should state that Marshall and Bischoff while failing to ever find the gyrus under consideration appearing at the surface, yet have identified it as concealed in the depths of the perpendicular fissure.

In the occipital lobe of an imbecile recently executed for murder at St. Louis, and whose brain was referred to me for examination, I have found the external occipital fissure perfect as in the embryo, though intersected by collateral fissures, and exhibiting a bevel, repeating to some extent its disposition in the anthropoid apes.

THE Albert Medal of the Society of Arts for 1879, was presented to Sir W. Thomson for his electrical researches, especially those relating to the transmission of telegraphic messages over ocean cables, last week, at a meeting of the council, held at Marlborough House. At the same time the Albert Medal for the current year was presented to James Prescott Joule, F. R. S., for the researches by which he established the true relation between heat, electricity, and mechanical work. The *conversazione* of the Society was also held last week, when Mr. Donald Currie, M.P., was presented with the Fothergill Gold Medal, the award of which we recently announced.

*The peduncular tracts of the anthropoid apes. *Journal of Nervous and Mental Diseases*, July, 1879.

DRY "MOUNTS" FOR THE MICROSCOPE.

BY PROFESSOR H. L. SMITH, HOBART COLLEGE, N. Y.

What shall we use to preserve dry mounts effectually? Many may think that nothing is easier; a cell of Brunswick black; a wax ring, or one of balsam; but the question is not thus easily to be disposed of. The writer has, within the last five years, mounted, or has had mounted under his supervision, some 15,000 slides of various microscopical objects, chiefly, however, foraminifera and diatoms; half of these were dry mounts.

Two things are important—the cell should be quickly and easily made, and the object when mounted in it should remain unchanged. There are very few cells as now made which will fulfil both these conditions, especially the latter. The deterioration of delicate dry mounts, and especially of test objects, sometimes within a few months after their preparation, but more or less certain in nearly every case, is well enough known.

All of the dry mounts of the Eulenstein series of diatoms, *e. g.*, which I have seen, are spoiled; and my cabinet is full of such preparations. Even Moller's do not escape, though they are, upon the whole, the most durable. I have abundance of amateur works that no doubt looked very beautiful just as they issued from the hands of the enthusiastic preparers, which are now, alas, mere wrecks; and worse than this, many choice and rare specimens, which I cannot replace, hopelessly ruined.

I believe that I was the first one to suggest the use of sheet wax for the bottoms of cells for foraminifera and other opaque objects, and of wax rings for diatoms and other transparent objects. (See Journal Quekett Club.)

The number of spoiled specimens, especially of diatoms and delicate transparent objects which I can now show, proves that this method of mounting is decidedly bad. I have lived to see the day when I shall be quite glad if the responsibility of suggesting such a nuisance as the wax ring can be transferred to some one else. For large opaque objects like most of the foraminifera, seeds, pollens, &c., the object itself is not so much injured, but the covering glass will, sooner or later, become covered (inside the cell), on the under surface, with a dew like deposit, which, when illuminated, will glisten almost like so many minute points of quicksilver, and though out of focus when the object is viewed, will show very disagreeably, like a thin gauze between; and with transparent objects these minute globules will not only dot the entire field, as so many dark or light points, but the object itself will appear as though it had been wetted.

Not long ago a well-known optician showed to me a spoiled slide "podura." The scales were very good and large—in fact, it was a slide which I had given to him, and it had been selected by myself in Beck's establishment in London as unexceptionably fine. This slide began slowly to show symptoms of "sweating." One scale after another

appeared as though moisture had, in some mysterious way, penetrated to the objects; it was not water, however, for when the cover, after much trouble, had been removed, and warmed sufficiently to evaporate anything like water, the scales still exhibited the same appearance, and, in fact, the heat required to get rid of this apparent moisture was so great, that the scales were charred. When wax rings are used, this apparent wetting or "sweating" occurs quickly, and more disagreeable than this, innumerable elongated specks, possibly crystalline, appear all over the under surface of the cover-glass. The same trouble occurs when any of the ordinary asphalt preparations are used, and the only cement which I have thus far found to be tolerably successful is shell-lac thoroughly incorporated with the finest carbon (diamond black) such as is used in the preparation of the best printing inks; the solvent being alcohol, these rings dry rapidly, and the cover is attached by heating. Even these rings cannot be trusted, unless thoroughly dry, and spontaneous drying is better than baking. I have had preparations spoiled after mounting on asphalt rings, which had been made for over a year, and which had been subjected for several hours to the heat of a steam bath. With large, somewhat coarse objects, the defect is not so marked, but with delicate ones, and especially test objects, it is simply a nuisance. With care I think the shell-lac rings may answer pretty well. I have not tried the aniline colored rings. The moisture (whatever it is), and the crystalline specks, appear to be derived from the vaporizable parts of the wax, or cement, given off under conditions where one would suppose such a thing impossible; it is however a fact; I have the proof of it, and I dare say hundreds of others have, too plainly evident.

There is another mode of making cells which promises well for permanence. My attention was first called to this method by Dr. Tulk, of London, who suggested for this purpose the thin gutta-percha tissue, used by surgeons in the place of oiled silk, I have had special punches made, which cut neat rings from this tissue, and I have used these rings with the greatest satisfaction. I have no preparations of my own more than about two years old; these so far, show no signs of change. Dr. Tulk informs me that he has them ten years old, and still good as when new. I have noticed that in some recent papers in the mineralogical journals the writers, who with little experience, have so lauded wax rings, speak of "thin rubber" for rings, evidently they have seen somewhere the gutta-percha mount, and supposed it rubber—the latter will not answer, melted rubber will not become hard. One beauty of the gutta percha ring is the very moderate heat required; it is thus available for many objects which might be injured by the greater heat necessary for the asphalt or shellac rings. As these rings in the arrangement which I have spoken of, can be rapidly made, and as they can be kept for any length of time (shut away from the dust), they are at any moment ready as well as convenient for use. The preparation is first arranged, dried or burnt on the cover, the slide cleaned, a ring laid on the centre and on this the cover is placed; the whole is now

held together by the forceps, and *slightly* warmed, just sufficient to soften the gutta percha; the forceps may now be laid aside, or used simply to press the cover home, warming the slide gently, also the cover; the perfect contact of the softened "tissue" with the cover and slide is easily recognized, and with a little care this can be effected very quickly, and nothing further is necessary. A finishing ring of colored cement makes a very neat mount, but it is not necessary.

ON MULTIPLE SPECTRA.

"Nunc age, quo motu genitalia materiali
Corpora res varias gignant, genitasque resolvant
Et qua vi facere id cogantur."

Lucretius ii., 61-2.

"Prima moventur enim per se primordia rerum:
Inde ea, quæ parvo sunt corpora conciliatu,
Et quasi proxima sunt ad vireis principiorum,
Ictibus illorum cæcis impulsa cientur
Ipsaque, quæ porro paulo maiora, lacesunt."

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"It is conceivable that the various kinds of matters, now recognized in different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement.

"The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."—*Graham's Researches*, p. 299.

In a recent paper* I showed that a study of the minute anatomy of spectra, both terrestrial and celestial, forces upon us the conclusion that both in the electric arc and in the hottest region of the sun the so-called chemical elements behave after the manner of compound bodies.

I then dealt more especially with the question of the basic lines in the various spectra, and it is clear that if at any one temperature, there be some lines only truly basic in the spectrum of any element, we at once divide the lines visible at that temperature into two groups, those which are basic and those which are not. This would give a compound origin to the lines, and this is the real point.

It is now years ago since the view was first held that the elementary bodies had double spectra, that is, that each, of at all events several, under changed conditions of temperature or electric tension, gave us now a fluted spectrum and now one composed of lines.

I glimpsed the idea some time afterward that the line spectrum was in its turn in all probability a complex whole, in other words that it was the summation of the spectra of various molecular groupings.

Recent work has to my mind not only shown that this is true, but that in the case of many bodies the complexity, and therefore the number, of the molecular groupings which give rise to that compound whole called a line spectrum, is considerable.

It is therefore important from my point of view to reconsider the evidence on which the assertion that the fluted bands and the line spectrum (taken as a whole) of a substance really belong to that substance, because if we find that this must be accepted and that it can easily be explained on the view that the two kinds of spectra are produced by different molecular groupings, the fact of other molecular groupings, giving rise to a complex line spectrum can be more readily accepted, contrary though it be to modern "chemical philosophy," as taught at all events in the text-books.

Plücker and Hittorf were, I believe, the first to point out that the same chemical substance, when in a state of gas

or vapor, gave out different spectra under different conditions. On this point they wrote fifteen years ago:

"The first fact which we discovered in operating with our tubes . . . was the following one:

"There is a certain number of elementary substances which, when differently heated, furnish two kinds of spectra of quite a different character, not having any line or any band in common."

"The fact is important, as well with regard to theoretical conceptions as to practical applications—the more so as the passage from one kind of spectrum to the other is by no means a continuous one, but takes place abruptly. By regulating the temperature you may repeat the two spectra in any succession *ad libitum*." (Plücker and Hittorf on the Spectra of Ignited Gases and Vapors: *Phil. Trans. Royal Society*, 1865, part i. p. 6.)

Angström, whose name must ever be mentioned with the highest respect by any worker in spectrum analysis, was distinctly opposed to this view, and in the text which accompanies his *Spectre Normal* we find the following statement:

"Dans un Mémoire sur les spectres 'doubles' des corps élémentaires que nous publierons prochainement, M. Thalen et moi, dans les Actes de la Société des Sciences d'Upsal, nous traiterons d'une manière suffisamment complète les questions importantes qu'on peut se proposer sur cet intéressant sujet. Pour le présent, je me borne à dire que les résultats auxquels nous sommes arrivés, ne confirment aucunement l'opinion émise par Plücker, qu'un corps élémentaire pourrait donner, suivant sa température plus ou moins élevée, des spectres tout-à-fait différents. C'est le contraire qui est exact. En effet en augmentant successivement la température, on trouve que les raies varient en intensité d'une manière très-compliquée, et que, par suite, de nouvelles raies peuvent même se présenter, si la température s'élève suffisamment. Mais, indépendamment de toutes ces mutations, le spectre d'un certain corps conservera toujours son caractère individuel."*

Angström did not object merely on theoretical grounds. He saw, or thought he saw, room to ascribe all these fluted spectra to impurities.

He was strengthened in this view by observing how, in the case of the spectra of known compounds, there were always flutings in one part of the spectrum or another; a rapid induction naturally, therefore, ascribed all flutings to compounds. The continuity of the gaseous and liquid states of matter, let alone the continuity of Nature's processes generally, never entered into the question. For Angström, as for the modern chemist, there was no such thing as evolution, no possibility of a close physical relationship between elements, so called, driven to incandescence from the solid state, and binary compounds of those elements.

In a memoir, however, which appeared after Angström's death, and which, though under a different title, was in all probability the one referred to, this opinion was to a large extent recalled, and in favor of Plücker's view, in the following words:—

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"Il est bien évident que les cas dont nous venons de parler, ne forment pas une exception à la loi générale énoncée ci-dessus, savoir que chaque corps simple ne peut donner qu'un seul spectre. En effet, si l'on suppose que l'état allotropique est dû à la constitution moléculaire du corps, soit que les molécules se combinent les unes avec les autres, soit qu'elles s'arrangent entre elles d'une certaine manière, cet état allotropique possèdera au point de vue spectroscopique, toutes les propriétés significatives d'un corps composé, et par conséquent il doit être décomposé de la même façon que celui-ci par les effets de la décharge disruptive de l'électricité."¹

I say that in this paper Angström recalled his own in favor of Plücker's view, because (as it has been remarked by Dr. Schuster²) the word "element" is used in a special sense—because in reality allotropic states are classed as compounds, that particular allotropic state which is to be regarded as truly elemental not being stated, nor any reason given why one should be thus singled out.

In the letter to which I have just referred Dr. Schuster gives an instance in which in order to show that elementary bodies did not really possess two spectra, a double spectrum was assigned to an acknowledged compound; the fluted spectra of hydrogen and carbon which differ from each other as widely as fluted spectra can, being both ascribed to acetylene.

Salet in his admirable work on the Spectra of the Metalloids,³ was driven to the conclusion that many of these bodies must be held to possess two spectra. His conclusions are thus expressed:—

"Nous avons comparé le spectre d'absorption du brome et de l'iode à leur spectre électrique, et cette comparaison nous semble mettre hors de doute la possibilité des spectres doubles. . . ."

"Nous avons obtenu, par voie électrique, un spectre primaire de l'iode correspondant à son spectre d'absorption. Le soufre, le sélénium et le tellure nous ont offert des spectres de combustion très-analogues aux spectres primaire obtenus par voie électrique, mais différant essentiellement des spectres des lignes. . . ."

"Nous avons produit le spectre primaire de l'azote avec différents corps qui n'ont absolument de commun que l'azote; nous pensons donc avoir démontré qu'il appartient bien réellement à ce métalloïde." (*Annales de Chimie et de Physique*, 4 série, tome xxviii. pp. 70, 71).

In 1868 Wullner⁴ gave his attention to this subject, and strongly supported Plücker's view of the existence of double spectra, indicating at the same time that the difference of temperature must be regarded as the sole cause of the phenomenon, adding, however, "a decomposition with further elements is not to be thought of." In the case of hydrogen he showed that the banded spectrum ascribed to acetylene really depended upon a change in the emissive power brought about by an alteration of temperature. Touching oxygen, he showed that three distinct spectra may be obtained, while in nitrogen two are observed.

I may say that in my early laboratory experiments I was at first led to think that, in the case of metallic vapors, Angström's first expressed opinion was correct, and I said so. But after more experience and knowledge had been acquired, I was compelled by the stern logic of facts to abandon it, and I showed, first, that more "orders" of spectra—to use Plücker's term—were necessary, and then that the line spectrum itself was in all probability compound; that is, that it was in some cases built up by the vibration of dissimilar molecules, some of which might even give us a fluted spectrum, if we could study them alone.

Although, however, in the views I have expressed on former occasions I have had the advantage of the support of the opinion of Plücker and Angström, and later of Dr. Schuster,² not to mention others, I am aware that though there is a general consensus among spectroscopic workers that double spectra cannot be ascribed to impurities, it is not absolute.

I propose therefore in this place to refer to a special case in which this question has been recently brought prominently forward.

I have already stated that Angström, who was the first to map the line-spectrum of carbon, ascribed the flutings ordinarily seen in the carbon compounds to acetylene.

Now Attfield, in 1862, as a result of a most carefully conducted and admirably-planned set of experiments, came to the conclusion that the flutings were really due to carbon: in short, that carbon, like hydrogen, iodine, sulphur, nitrogen, and other bodies, had a fluted spectrum as well as one consisting wholly of lines.

The work of Attfield will be gathered from the following extract from his paper (*Phil. Trans.*, vol. clii. part i, p. 221 et seq.):—

"On recently reading Swan's paper by the light that Professors Bunsen and Kirchhoff have thrown on the subject, I came to the conclusion that these bands must be due to the incandescent carbon vapor; that, if so, they must be absent from flames in which carbon is absent, and present in flames in which carbon is present; that they must be observable equally in the flames of the oxide, sulphide, and nitride as in that of the hydride of carbon; and, finally, that they must be present whether the incandescence be produced by the chemical force, as in burning jets of the gases in the open air, or by the electric force, as when hermetically-sealed tubes of the gases are exposed to the discharge of a powerful induction-coil. . . ."

"To establish the absolute identity of the hydro- and nitro-carbon spectra, excluding of course the lines due to nitrogen, they were simultaneously brought into the field of the spectroscope: one occupying the upper, and the other the lower half of the field.

"This was readily effected after fixing the small prism, usually supplied with spectroscopes, over half of the narrow slit at the further end of the object-tube of the instrument. The light from the oxyhydrocarbon flame was now directed up the axis of the tube by reflection from the little prism, while that from the oxynitrocarbon flame passed directly through the uncovered half of the slit. A glance through the eye-tube was sufficient to show that the characteristic lines of the hydrocarbon spectrum were perfectly continued in the nitrocarbon spectrum. A similar arrangement of apparatus, in which the hydrocarbon light was replaced by that of pure nitrogen, showed that the remaining lines of the nitrocarbon spectrum were identical with those of the nitrogen spectrum. In this last experiment the source of the pure nitrogen light was the electric discharge through the rarefied gas.

"The above experiment certainly seemed to go far towards proving the spectrum in question to be that of the element carbon. Nevertheless, the ignition of the gases having been effected in air, it was conceivable that hydrogen, nitrogen, or oxygen had influenced the phenomena. To eliminate this possible source of error, the experiments were repeated out of contact with air. A thin glass tube 1 inch in diameter and 3 inches long, with platinum wires fused into its sides, and its ends prolonged by glass quills, having a capillary bore, was filled with pure dry cyanogen and the greater portion of this gas then removed by a good air-pump. Another tube was similarly prepared with olefiant gas. The platinum wires in these tubes were then so connected with each other that the electric discharge from a powerful induction-coil could pass through both at the same time. On now observing the spectra of these two lights in the simultaneous manner previously described, the characteristic lines of the hydrocarbon spectrum were found to be rigidly continued in that of the nitrocarbon. Moreover, by the same method of simultaneous observation, the spectrum of each of these electric flames, as they

¹ Dr. Schuster's recently published investigations are as follows:—

Mr. Lockyer's investigations have shown that most bodies give us a continuous spectrum, as a gas, before they condense, and many at a considerable temperature above the boiling point. Mr. Lockyer has rightly drawn the conclusion from these facts, that the atomic aggregation of the molecules is the cause of the different orders of spectra.

That the discontinuous spectra of different orders (line and band spectra) are due to different molecular combinations, I consider to be pretty well established, and analogy has led me (and Mr. Lockyer before me) to explain the continuous spectra by the same cause; for the change of the continuous spectrum to the line or band-spectrum takes place in exactly the same way as the change of spectra of different orders into each other. Analogy is not a strong guide, yet some weight may be given to it in a case like the one under discussion, where experiment hitherto has failed to give a decided answer. (Dr. A. Schuster on the Spectra of Metalloids, *Phil. Trans. Royal Society*, 1879. Part i. page 38 and 89, note).

² Angström and Thalén's "Recherches sur les Spectres des Métalloïdes," p. 5.

³ NATURE, vol. xv. p. 447.

⁴ *Ann. de Chimie et de Physique*, 1873, vol. xxviii. p. 1.

⁵ *Phil. Mag.*, sec. 4, vol. xxxvii. p. 405.

may be termed, was compared with the corresponding chemical flames, that is, with the oxhydrocarbon and oxynitrocarbon jets of gas burning in air. The characteristic lines were present in every case. Lastly, by similar inter-observation a few other lines in the electric spectrum of the hydrocarbon were proved to be due to the presence of hydrogen, and several others in the electric spectrum of the nitrocarbon to be caused by the presence of nitrogen.

"The spectrum under investigation having then been obtained in one case when only carbon and hydrogen were present, and in another when all elements but carbon and nitrogen were absent, furnishes to my mind, sufficient evidence that the spectrum is that of carbon."

"But an interesting confirmation of the conclusion just stated is found in the fact that the same spectrum is obtained when no other elements but carbon and oxygen are present, and also when carbon and sulphur are the only elements under examination. And first with regard to carbon and oxygen. Carbonic oxide burned in air gives a flame possessing a continuous spectrum. A mixture of carbonic oxide and oxygen burned from a platinum-tipped safety-jet also gives a more or less continuous spectrum, but the light of the spectrum has a tendency to group itself in ill-defined ridges. Carbonic oxide, however, ignited by the electric discharge in a semi-vacuous tube, gives a bright sharp spectrum. This spectrum was proved, by the simultaneous method of observation, to be that of carbon plus the spectrum of oxygen. With regard to carbon and sulphur almost the same remarks may be made. Bisulphide of carbon vapor burns in air with a bluish flame. Its spectrum is continuous. Mixed with oxygen and burned at the safety-jet, its flame still gives a continuous spectrum, though more distinctly furrowed than in the case of carbonic oxide; but when ignited by the electric current its spectrum is well defined, and is that of carbon plus the sulphur. That is to say, it is the spectrum of carbon plus the spectrum that is obtained from vapor of sulphur when ignited by the electric discharge in an otherwise vacuous tube."

"Having thus demonstrated that dissimilar compounds containing carbon emit, when sufficiently ignited, similar rays of light, I come to the conclusion that those rays are characteristic of ignited carbon vapor, and that the phenomena they give rise to on being refracted by a prism is the spectrum of carbon."

This question was next taken up by Morren. He wrote¹ (in 1865) fifteen years ago:

"A la réception de cet intéressant et substantiel Mémoire, j'avoue que je ne regardai pas d'abord comme fondée l'assertion de M. Attfield.

"Je me suis donc mis au travail avec la pensée préconçue de combattre l'assertion anglaise par le savant anglais; mais pas du tout, il résulte au contraire des expériences auxquelles je me suis livré que M. Attfield a raison, et que c'est bien la vapeur du carbone qui donne le spectre indiqué plus haut.

"Si on fait brûler le cyanogène au moyen du chalumeau à deux courants, en faisant arriver au centre de la flamme du cyanogène un courant d'oxygène très-pur (cette condition est indispensable), on voit se produire un des plus beaux effets de combustion possible, et cette expérience est certainement une des plus magnifiques qu'on puisse réaliser sur la combustion des gaz. Il se produit, au milieu de la flamme rosé-violet de la cyanogène, une boule d'un blanc vert éblouissant qui rappelle la lumière électrique produite par le courant de la pile entre deux charbons de corne. Si le spectroscopie est dirigé sur cette brillante lumière, on aperçoit, avec une splendeur merveilleuse, le même spectre de la partie bleue des flammes hydrocarbonées. Ainsi donc c'est du charbon seul, mais à l'état de vapeur, qui forme cette boule brillante qui plus loin, par son union avec l'oxygène, va passer à l'état d'acid carbonique. Du reste ce spectre n'est pas seul; avec lui on voit, mais très-effacé, le spectre spécial du cyanogène, et celui-ci tend de plus en plus à disparaître à mesure que l'oxygène arrive avec plus d'abondance et brûle de mieux en mieux le

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Four years later Dr. Watts devoted himself to this subject, and in 1869 his work was thus summarized by himself:

"This spectrum [that consisting of the flutings in question] may be obtained from the flame of any hydrocarbon, though in many cases, owing to the faintness of the spectrum, only some of the groups can be recognized. In the flame of an ordinary Bunsen burner δ and ϵ are easily seen, γ and f are much fainter, and the red group can not be detected.

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These data relating to visibility must, doubtless, be regarded in direct connection with the numerical aperture of the objectives of the illumination, as pointed out by Mr. Stephenson. He gave practical demonstrations of the views explained in his paper by exhibiting several slides mounted in the different media. I mention one slide of *Pleurosigma Elongatum*, mounted in sol. of phosphorus in bisulphide of carbon, as presenting to the eye the strongest image that has come under my notice. According to Mr. Stephenson's theory, the visibility under these conditions would be about six times as great as that of the same object mounted in balsam. Is it possible to induce our professional object-mounters to take up the subject? Surely there are many amateurs of fine definition who would like to see the conditions of visibility pushed to the highest point, and who would amply repay the modicum of exertion needed to produce them.

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SCIENCE :

A WEEKLY RECORD OF SCIENTIFIC
PROGRESS.

JOHN MICHELS, Editor.

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To Correspondents.

All communications should be addressed to the Editor—Box 3838, P. O., New York—with name and address of writer, not necessarily for publication without consent.

Scientific papers and correspondence intended for publication, should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply, may be written in the form of an article.

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CO-OPERATION IN SCIENCE.

At this season of rest and recreation, naturalists are now wandering on many a hill and dale or upon the breezy shore, intent on collecting specimens which shall employ their leisure hours in the Autumn and coming Winter. From the close precincts of a city, who would not be free and join the little band, who with nets, bottles, corks and pins, are now seeking things bright and rare, which Nature has so lavishly scattered, and yet with such a cunning hand, that even the search and gathering affords delight to those who diligently seek.

The number of collectors is steadily increasing, but when we contemplate the immense ground to be covered, the necessity for increasing the force becomes but too apparent. Not only have the depths of mighty oceans to be explored, and the bottom of great seas investigated; but also tiny denizens of little rock pools must be described and classified. The surface of the

earth might seem too immense to be minutely examined by the naturalist, but still he has to descend many hundred feet into the bowels of the earth to there find records of past life, so as to complete the great catalogue of Nature's works.

Earth, air, and water have thus to be surveyed, and still another world of life and form which is invisible to the natural vision of man. These microscopic forms are not to be neglected, for they decimate populations and destroy the industry of nations, and are so numerous, that the accumulation of their countless numbers rear up mighty ranges of mountains.

With such a work before collectors who are hopefully struggling to add to our garner of a knowledge of Nature, who cannot desire that their number may be increased. Yet how easily it might be done. From the fact that some of our best collectors are not scientific men, it would seem that a convenient division of labor is here suggested, namely: the amateur who collects, and the scientist who describes and classifies. Some men possess special qualifications for collecting, they discover localities which others would never dream of, and they have a happy knack of always finding what they are in search of. Such expertness may be largely due to constant practice, but the professional scientist finds himself tied to his study and laboratory, and has no time for these constant perambulations; he must therefore largely rely on the amateur collector for his material, as the latter must depend on him, for his technical and scientific knowledge.

If therefore, there are two classes who are dependent upon each other for the extension of a great and valuable work, let that co-operation be more distinctly recognized and accepted, for such is far from being the case at the present time, particularly in the United States, where of all places it might be expected to thrive.

One of the best means of organizing an efficient corps of amateur workers, is the establishment of local scientific societies, and the circulation of moderate priced but strictly first-class scientific journals.

In Europe, and especially England, scientific societies, chiefly supported by amateurs, are now doing excellent work; but in the United States (with a few exceptions) they drag on a miserable existence of poverty and inutility. What is the cause of this? Simply a want of co-operation between the amateur and professional scientist, and a due regard for each other's position.

To be effective, the officers and council of a scientific society should be taken from the best professional scientists in the neighborhood—men able to command and organize the work of the society, and give a

character and tone to its proceedings. The amateurs must constitute the rank and file, accept that position, and keep to it, until by some special qualifications they may be promoted to a higher grade.

In England such is the case. Taking the case of the Quekett Microscopical Society, such men as Professor Huxley, Dr. Lionel Beale and Dr. Cobbold, the eminent helminthologist, have presided over the proceedings, and the result has been that over five hundred members have enlisted under such leadership. Compare this with the American Microscopical Society of New York established in the same year. Who ever heard of a paper read before this body, or a single piece of scientific work performed by one of its members? No fossil could be more inactive than this society; it exists on paper only, and for the benefit of a few officials.

A younger Microscopical Society, established in New York city about three years since, has been organized on an equally faulty basis, and now numbers but thirty members. The co-operation of the right men has never been asked, and probably would not be accepted, and in consequence, a future of inactivity and embarrassment may be anticipated.

To make American Scientific Societies as effective as those in England, they must be organized on a sufficiently popular basis, to interest the sympathy and support of the public; and presided over by men of known scientific ability, whose presence will encourage the student, and give a character to the proceedings.

In regard to the aid given by scientific journals in promoting useful co-operation between the scientist and the student, we may state that one of the objects of "SCIENCE" is to promote such a consummation, and that aim will be constantly kept in view. As a step in the right direction we have here indicated some of the means, by which the icy barrier which now separates those who should be cordially united in a great work, may be gently thawed by the inspiring influences of united action and generous co-operation.

A SCHOOL of agriculture has been formed at Canterbury, New Zealand, situated at Lincoln, twelve miles from the city of Canterbury. This institution is under the direction of Mr. W. E. Ivey, comprises lecture theatre, library, museum, chemical laboratory. A farm of 500 acres is attached to the institution, a portion of which will be devoted to experimental purposes for testing the various methods of cultivation.

M. DAUBREE, director of the French School of Mines, has published an essay on Descartes, in which he summarises the services rendered by that philosopher to science. He reminds his readers that Descartes advocated the theory of an igneous origin for the earth, and he enters into a lengthened discussion of the objections which may be raised against the theory of actual causes.

THE Earl of Spencer, in a recent speech in the House of Lords, *admitted* the application of science to agriculture. He said: "Great attention had of late years been very properly called to the great aid which science gave to the various classes of manufactures and producers; and that principle applied with quite as great force to agriculture as to any other art. If science could enable our agriculturists to produce more from the land than they had hitherto done, it would add another to the many useful things it had been the means of accomplishing." These words might, with some propriety, have been spoken twenty years ago, and if they represent the present relation of science to British agriculture, much of the unprofitable results of farming in that country may be thus explained.

LORD Spencer said, that it had been at last decided to open a class for agriculture next August, at the Department of Science and Art. We commend Lord Spencer to a perusal of the reports of the department of Agriculture at Washington, especially that for 1878, in which the value of science to the agriculturist is very evident.

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At a recent trial in England, a gas company was sued for damages, the plaintiff having been rendered insensible by an escape of the company's gas, due to a breakage in their mains. The plaintiff alleged that he suffered for a considerable length of time after the accident, and was unfit for business. The jury accepted the view of Dr. Tidy and Dr. Hastings, who gave scientific evidence on the subject, they being of the opinion that the effect of inhaling coal gas was very transitory; and that if sufficient was not inhaled to cause death, it would shortly pass from the system, and its ill effects cease.

CORRESPONDENCE.

To the Editor of Science:

DEAR SIR:—In the Physical Laboratory we noticed last Winter a beautiful experiment with vapors. An alcohol lamp, burning, was put under the receiver of the air pump. A few strokes put out the flame. The air returning, a single stroke of the piston caused the receiver to fill with a dense and transient cloud, soon disappearing with a change of pressure in the receiver. This experiment has interesting relations to rain fall, and other meteorological phenomena.

G. M. MANSFIELD.

*Laboratory of Asbury University,
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To the Editor of Science:

DEAR SIR:—In the Physical Laboratory we noticed last Winter a beautiful experiment with vapors. An alcohol lamp, burning, was put under the receiver of the air pump. A few strokes put out the flame. The air returning, a single stroke of the piston caused the receiver to fill with a dense and transient cloud, soon disappearing with a change of pressure in the receiver. This experiment has interesting relations to rain fall, and other meteorological phenomena.

G. M. MANSFIELD.

Laboratory of Asbury University.
Indiana, July 7, 1880.

character and tone to its proceedings. The amateurs must constitute the rank and file, accept that position, and keep to it, until by some special qualifications they may be promoted to a higher grade.

In England such is the case. Taking the case of the Quekett Microscopical Society, such men as Professor Huxley, Dr. Lionel Beale and Dr. Cobbold, the eminent helminthologist, have presided over the proceedings, and the result has been that over five hundred members have enlisted under such leadership. Compare this with the American Microscopical Society of New York established in the same year. Who ever heard of a paper read before this body, or a single piece of scientific work performed by one of its members? No fossil could be more inactive than this society; it exists on paper only, and for the benefit of a few officials.

A younger Microscopical Society, established in New York city about three years since, has been organized on an equally faulty basis, and now numbers but thirty members. The co-operation of the right men has never been asked, and probably would not be accepted, and in consequence, a future of inactivity and embarrassment may be anticipated.

To make American Scientific Societies as effective as those in England, they must be organized on a sufficiently popular basis, to interest the sympathy and support of the public; and presided over by men of known scientific ability, whose presence will encourage the student, and give a character to the proceedings.

In regard to the aid given by scientific journals in promoting useful co-operation between the scientist and the student, we may state that one of the objects of "SCIENCE" is to promote such a consummation, and that aim will be constantly kept in view. As a step in the right direction we have here indicated some of the means, by which the icy barrier which now separates those who should be cordially united in a great work, may be gently thawed by the inspiring influences of united action and generous co-operation.

A SCHOOL of agriculture has been formed at Canterbury, New Zealand, situated at Lincoln, twelve miles from the city of Canterbury. This institution is under the direction of Mr. W. E. Ivey, comprises lecture theatre, library, museum, chemical laboratory. A farm of 500 acres is attached to the institution, a portion of which will be devoted to experimental purposes for testing the various methods of cultivation.

M. DAUBREE, director of the French School of Mines, has published an essay on Descartes, in which he summarises the services rendered by that philosopher to science. He reminds his readers that Descartes advocated the theory of an igneous origin for the earth, and he enters into a lengthened discussion of the objections which may be raised against the theory of actual causes.

THE Earl of Spencer, in a recent speech in the House of Lords, *admitted* the application of science to agriculture. He said: "Great attention had of late years been very properly called to the great aid which science gave to the various classes of manufactures and producers; and that principle applied with quite as great force to agriculture as to any other art. If science could enable our agriculturists to produce more from the land than they had hitherto done, it would add another to the many useful things it had been the means of accomplishing." These words might, with some propriety, have been spoken twenty years ago, and if they represent the present relation of science to British agriculture, much of the unprofitable results of farming in that country may be thus explained.

LORD Spencer said, that it had been at last decided to open a class for agriculture next August, at the Department of Science and Art. We commend Lord Spencer to a perusal of the reports of the department of Agriculture at Washington, especially that for 1878, in which the value of science to the agriculturist is very evident.

THE value of scientific journals has been attested to, by the humble class of astronomers who exhibit their telescopes at corners of streets in Paris, showing the moon, planets and other celestial objects which may be seen with telescopes of moderate quality. They state that since the publication of the *Astronomie Populaire* the number of their customers has nearly doubled.

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A DARWINIAN STUDY.

BY ALFRED R. WALLACE.

For the benefit of those unacquainted with entomology we may state, that many butterflies have two, or even three broods in a year. One brood appears in spring, their larvæ having fed during the preceding autumn, and passed the winter in the pupa state, while the others appear later in the year, having passed rapidly through all their transformations and thus never having been exposed to the cold of winter. In most cases the insects produced under these opposite conditions present little or no perceptible difference; but in others there is a constant variation, and sometimes this is so great that the two forms have been described as distinct species. The most remarkable case among European butterflies is that of *Araschnia proorsa*, the winter or spring form of which was formerly considered to be a distinct species and named *Araschnia levana*. The two insects differ considerably in both sexes, in markings, in color, and even in the form of the wings, so that till they were bred and found to be alternate broods of the same species (about the year 1830) no one doubted their being altogether distinct.

In order to learn something of the origin and nature of this curious phenomenon Dr. Weismann has for many years carried on a variety of experiments, breeding the species in large numbers and subjecting the pupæ to artificial heat or cold for the purpose of hastening or retarding the transformation. The result of these experiments is, that by subjecting the summer brood to severe artificial cold in the pupa state, it may be made to produce perfect insects the great majority of which are of the winter form, but, on the other hand, no change of conditions that has yet been tried has any effect in changing the winter to the summer form. Taking this result in connection with the fact that in high latitudes where there is only one brood a year it is always the winter form, Dr. Weismann was led to the hypothesis that this winter form was the original type of the species, and that the summer form has been produced gradually, since the glacial epoch, by the summer becoming longer and thus admitting of the production of a second or summer brood. This explains why the production of the winter form (*A. levana*) from summer larvæ is easy, it being a reversion to the ancestral type; while the production of the summer form (*A. proorsa*) from autumnal larvæ is impossible, because that form is the result of gradual development; and processes of development which have taken thousands of years to bring about cannot be artificially reproduced in a single season.

This hypothesis was supported by experiments with another two-brooded species, *Pieris napi*, with similar results, the winter form being produced with certainty by the application of cold to summer pupæ; and Mr. Edwards, in America, has made similar experiments with the various forms of *Papilio ajax*, finding that the summer broods can be changed into the winter form by the application of cold, while the winter broods can never be made to assume the summer form by hastening the process of transformation. In the Arctic regions and in the high Alps there is only one form of *Pieris napi*, which very closely resembles the winter form of the rest of Europe, and this could never be the least changed by rapidly developing the pupæ under the influence of heat.

Another curious case is that of one of the *Lycenidæ* (*Plebeius agestis*) which exhibits three forms, which may be designated as A, B, and C. The first two, A and B, are alternate broods (winter and summer) in Germany, while in Italy the corresponding forms are B and C, so that B is the summer form in Germany and the winter form in Italy. Here we see climatic varieties in process of formation in a very curious way.

That temperature during the pupa stage is a very powerful agent in modifying the characters of butterflies, is well shown by the case of *Polyommatus phleas*. The two broods of this insect are alike in Germany, while in Italy the summer brood has the wings dusky instead of copper-colored. The period of development is exactly the same in both countries, so that the change must, it is argued, be attribut-

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Dr. Weismann lays great stress on the varied effects of temperature in modifying allied species or the two sexes of the same species, from which he argues that the essential cause of all these changes is to be found in peculiarities of physical constitution, which cause different species, varieties, or sexes to respond differently to the same change of temperature; and he thinks that many sexual differences can be traced to this cause alone without calling in the aid of sexual selection. The general result arrived at by the laborious investigation of these phenomena is, that—"a species is only caused to change through the influence of changing external conditions of life, this change being in a fixed direction which entirely depends on the physical nature of the varying organism, and is different in different species, or even in the two sexes of the same species;" and he adds:—"According to my view, transmutation by purely internal causes is not to be entertained. If we could absolutely suspend the changes of the external conditions of life, existing species would remain stationary. The action of external inciting causes, in the widest sense of the word, is alone able to produce modifications; and even the never-failing 'individual variations,' together with the inherited dissimilarity of constitution, appear to me to depend upon unlike external influences, the inherited constitution itself being dissimilar, because the individuals have been at all times exposed to somewhat varying external influences." The present writer has arrived at almost exactly similar conclusions to these, from a study of the geographical distribution and specific variation of animal forms, as stated in an article on "The Origin of Species and Genera," which appeared in the *Nineteenth Century* of January last, and it is gratifying to find them supported by the results of a very different line of inquiry, and by the authority of so eminent and original an observer as Dr. Weismann.

A FOURTH STATE OF MATTER¹

In introducing the discussion on Mr. Spottiswoode and Mr. Moulton's paper on the "Sensitive State of Vacuum Discharges," at the meeting of the Royal Society on April 15, Dr. De La Rue, who occupied the chair, good-naturedly challenged me to substantiate my statement that there is such a thing as a fourth or ultra-gaseous state of matter.

I had no time then to enter fully into the subject; nor was I prepared, on the spur of the moment, to marshal all the facts and reasons which have led me to this conclusion. But as I find that many other scientific men besides Dr. De La Rue are in doubt as to whether matter has been shown to exist in a state beyond that of gas, I will now endeavor to substantiate my position.

I will commence by explaining what seems to me to be the constitution of matter in its three states of solid, liquid, and gas.

I. First as to Solids:—These are composed of discontinuous molecules, separated from each other by a space which is relatively large—possibly enormous—in comparison with the diameter of the central nucleus we call *molecule*. These molecules, themselves built up of *atoms*, are governed by certain forces. Two of these forces I will here refer to—attraction and motion. Attraction when exerted at sensible distances is known as *gravitation*, but when the distances are molecular it is called *adhesion* and *cohesion*. Attraction appears to be independent of absolute temperature; it increases as the distance between the molecules diminishes; and were there no other counteracting force the result would be a mass of molecules in actual contact, with no molecular movement whatever—a state of things beyond our conception—a state, too, which would probably result in the creation of something that, according to our present views would not be *matter*.

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The state of solidity, therefore—the state which we are in the habit of considering *par excellence* as that of *matter*—is merely the effect on our senses of the motion of the discrete molecules among themselves.

Solids exist of all consistencies, from the hardest metal, the most elastic crystal, down to thinnest jelly. A perfect solid would have no viscosity, *i.e.*, when rendered discontinuous or divided by the forcible passage of a harder solid, it would not close up behind and again become continuous.

In solid bodies the cohesion varies according to some unknown factor which we call chemical constitution; hence each kind of solid matter requires raising to a different temperature before the oscillating molecules lose their fixed position with reference to one another. At this point, varying in different bodies through a very wide range of temperature, the solid becomes liquid.

II. In liquids the force of cohesion is very much reduced, and the adhesion or the fixity of position of the centres of oscillating molecules is destroyed. When artificially heated, the inter-molecular movements increase in proportion as the temperature rises, until at last cohesion is broken down, and the molecules fly off into space with enormous velocities.

Liquids possess the property of viscosity—that is to say, they offer a certain opposition to the passage of solid bodies; at the same time they cannot permanently resist such opposition, however slight, if continuously applied. Liquids vary in consistency from the hard, brittle, apparently solid pitch to the lightest and most ethereal liquid capable of existing at any particular temperature.

The state of liquidity, therefore, is due to inter-molecular motions of a larger and more tumultuous character than those which characterize the solid state.

III. In gases the molecules fly about in every conceivable direction, with constant collision and enormous and constantly varying velocities, and their mean free path is sufficiently great to release them from the force of adhesion. Being free to move, the molecules exert pressure in all directions, and were it not for gravitation they would fly off into space. The gaseous state remains so long as the collisions continue to be almost infinite in number, and of inconceivable irregularity. The state of gaseity, therefore, is pre-eminently a state dependent on collisions. A given space contains millions of millions of molecules in rapid movement in all directions, each molecule having millions of encounters in a second. In such a case the length of the mean free path of the molecules is exceeding small compared with the dimensions of the containing vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed.

What, then, are these molecules? Take a single lone molecule in space. Is it solid, liquid, or gas? Solid it cannot be, because the idea of solidity involves certain properties which are absent in the isolated molecule. In fact, an isolated molecule is an inconceivable entity, whether we try, like Newton, to visualise it as a little hard spherical body, or, with Bosovich and Faraday, to regard it as a centre of force, or accept Sir William Thomson's vortex atom. But if the individual molecule is not solid, *à fortiori* it cannot be regarded as a liquid or gas, for these states are even more due to inter-molecular collisions than is the solid state. The individual molecules, therefore, must be classed by themselves in a distinct state or category.

The same reason applies to two or to any number of contiguous molecules, provided their motion is arrested or controlled, so that no collisions occur between them; and even supposing this aggregation of isolated non-colliding molecules to be bodily transferred from one part of space to

another, that kind of movement would not thereby cause this molecular collocation to assume the properties of gas; a molecular wind may still be supposed to consist of isolated molecules, in the same way as the discharge from a mitrailleuse consists of isolated bullets.

Matter in the fourth state is the ultimate result of gaseous expansion. By great rarefaction the free path of the molecules is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motion or laws without interference; and if the mean free path is compatible with the dimensions of the containing vessel, the properties which constitute gaseity are reduced to a minimum, and the matter then becomes exalted to an ultra-gaseous state.

But the same condition of things will be produced if by any means we can take a portion of gas, and by some extraneous force infuse order into the apparently disorderly jostling of the molecules in every direction, by coercing them into a methodical rectilinear movement. This I have shown to be the case in the phenomena which cause the movements of the radiometer, and I have rendered such motion visible in my later researches on the negative discharge in vacuum tubes. In the one case the heated lamp-black and in the other the electrically excited negative pole supplies the *force majeure* which entirely or partially changes into a rectilinear motion the irregular vibration in all directions; and according to the extent to which this onward movement has replaced the irregular motions which constitute the essence of the gaseous condition, to that extent do I consider that the molecules have assumed the condition of radiant matter.

Between the third and the fourth states there is no sharp line of demarcation, any more than there is between the solid and liquid states, or the liquid and gaseous states; they each merge insensibly one into the other. In the fourth state properties of matter which exist even in the third state are shown *directly*, whereas in the state of gas they are only shown *indirectly*, by viscosity and so forth.

The ordinary laws of gases are a simplification of the effects arising from the properties of matter in the fourth state; such a simplification is only permissible when the mean length of path is small compared with the dimensions of the vessel. For simplicity's sake we make abstraction of the individual molecules, and feign to our imagination *continuous* matter, of which the fundamental properties—such as pressure varying as the density, and so forth—are ascertained by experiment. A gas is nothing more than an assembly of molecules contemplated from a simplified point of view. When we deal with phenomena in which we are obliged to contemplate the molecules individually, we must not speak of the assemblage as *gas*.

These considerations lead to another and curious speculation. The molecule—intangible, invisible, and hard to be conceived—is the only true *matter*, and that which we call matter is nothing more than the effect upon our sense of the movements of molecules, or, as John Stuart Mill expresses it, “a permanent possibility of sensation.” The space covered by the motion of molecules has no more right to be called matter than the air traversed by a rifle bullet can be called lead. From this point of view, then, matter is but a mode of motion; at the absolute zero of temperature the inter-molecular movement would stop, and although *something* retaining the properties of inertia and weight would remain, *matter*, as we know it, would cease to exist.

NOTE BY THE DUKE OF ARGYLE.

In the very interesting communication from Mr. Crookes on “A Fourth State of Matter,” which is contained in *Nature*, vol. xxii. p. 153, there is a paragraph at the end which advances, as it seems to me, some most disputable propositions.

Like many other questions of modern science, the question he raises is to a very large extent a question of definition. But questions of definition are questions of the very highest importance in philosophy, and they need to be watched accordingly.

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But the same condition of things will be produced if by any means we can take a portion of gas, and by some extraneous force infuse order into the apparently disorderly jostling of the molecules in every direction, by coercing them into a methodical rectilinear movement. This I have shown to be the case in the phenomena which cause the movements of the radiometer, and I have rendered such motion visible in my later researches on the negative discharge in vacuum tubes. In the one case the heated lamp-black and in the other the electrically excited negative pole supplies the *force majeure* which entirely or partially changes into a rectilinear motion the irregular vibration in all directions; and according to the extent to which this onward movement has replaced the irregular motions which constitute the essence of the gaseous condition, to that extent do I consider that the molecules have assumed the condition of radiant matter.

Between the third and the fourth states there is no sharp line of demarcation, any more than there is between the solid and liquid states, or the liquid and gaseous states; they each merge insensibly one into the other. In the fourth state properties of matter which exist even in the third state are shown *directly*, whereas in the state of gas they are only shown *indirectly*, by viscosity and so forth.

The ordinary laws of gases are a simplification of the effects arising from the properties of matter in the fourth state; such a simplification is only permissible when the mean length of path is small compared with the dimensions of the vessel. For simplicity's sake we make abstraction of the individual molecules, and feign to our imagination *continuous* matter, of which the fundamental properties—such as pressure varying as the density, and so forth—are ascertained by experiment. A gas is nothing more than an assembly of molecules contemplated from a simplified point of view. When we deal with phenomena in which we are obliged to contemplate the molecules individually, we must not speak of the assemblage as *gas*.

These considerations lead to another and curious speculation. The molecule—intangible, invisible, and hard to be conceived—is the only true *matter*, and that which we call matter is nothing more than the effect upon our sense of the movements of molecules, or, as John Stuart Mill expresses it, “a permanent possibility of sensation.” The space covered by the motion of molecules has no more right to be called matter than the air traversed by a rifle bullet can be called lead. From this point of view, then, matter is but a mode of motion; at the absolute zero of temperature the inter-molecular movement would stop, and although *something* retaining the properties of inertia and weight would remain, *matter*, as we know it, would cease to exist.

NOTE BY THE DUKE OF ARGYLE.

In the very interesting communication from Mr. Crookes on “A Fourth State of Matter,” which is contained in *Nature*, vol. xxii. p. 153, there is a paragraph at the end which advances, as it seems to me, some most disputable propositions.

Like many other questions of modern science, the question he raises is to a very large extent a question of definition. But questions of definition are questions of the very highest importance in philosophy, and they need to be watched accordingly.

Speculating on the ultimate conceptions of Matter which are affected by the discovery of it in "a fourth condition," Mr. Crookes says: "From this point of view, then, Matter is but a 'mode of motion.'"

It has never appeared to me that this well-known phrase is a very happy one, even as applied to Heat. It is possible, of course, to consider Heat from this point of view. But then it is equally possible to consider all other phenomena whatever from the same point of view. Not only Heat, but Light, Sound, Electricity, Galvanism, and Sensation itself in all its forms, may be regarded as "modes of motion."

But at least in the application of this phrase to Heat there is an intelligible meaning, and not a mere confusion of thought. But as applied to Matter—as a definition of our ultimate conception of matter—it appears to me to confound distinctions which are primary and essential. "Motion" is an idea which presupposes Matter and Space. Motion has no meaning whatever except the movement of Matter in Space. To define Matter, therefore, as a "mode of motion," is to define it as Matter in a state of motion. But this definition necessarily implies that Matter can also be conceived as without motion, and accordingly Mr. Crookes is obliged to confess that "at the absolute zero of temperature inter-molecular movement would stop," and that after that, Matter would remain with all the "properties of inertia and of weight."

Again Mr. Crookes says: "The space covered by the motion of molecules has no more right to be called Matter than the air traversed by a rifle bullet can be called lead." No doubt this is true; but it implies what is not true, that the common idea of Matter is nothing but "the space covered by the motion of molecules." The popular idea attached to words of primary significance may not be always adequate or complete. But in my opinion they are generally much more near the truth, and more accurately represent the truth than most of the phrases which scientists are now inventing in the region of transcendental physics.

These phrases have their value and their interest as representing special and partial aspects of phenomena. But I hold that the unconscious metaphysics of human speech are often the deepest and truest interpretations of the ultimate facts of nature

ON A NEW JELLY-FISH OF THE ORDER TRACHOMEDUSÆ, LIVING IN FRESH WATER.

On Thursday last, June 10, Mr. Sowerby, the Secretary of the Botanical Society of London, observed in the tank in the water-lily house in Regent's Park a peculiar organism, of which he was kind enough to place a large number at my disposal on the following Monday.

The organism proves to be an adult medusa belonging to the order Trachomedusæ and the family Petasidæ of Hæckel's system ("System der Medusen," Erster Theil). It comes nearest among described genera to Fritz Müller's imperfectly known *Aglauropsis* from the coast of Brazil.

The most obviously interesting matter about the form under notice is that it occurs in great abundance in perfectly fresh water at a temperature of 90° Fahr.

Hitherto no medusa of any order has been detected in fresh water—except perhaps some stray estuarine forms (*Crambessa*?).

It is exceedingly difficult to trace the introduction of this animal into the tank in the Regent's Park, since no plants have been recently (within twelve months) added to the lily-house, and the water is run off every year. Probably a few specimens were last year or the year before present in the tank, and have only this year multiplied in sufficient abundance to attract attention. Clearly this medusa is a tropical species, since it flourishes in water of the high temperature of 90° Fahr.

Mr. Sowerby has observed the medusa feeding on *Daphnia*, which abounds in the water with it.

The present form will have to be placed in a new genus, for which I propose the name *Craspedacusta*, in allusion to the relation of its otocysts to its velum.

It is one of the sub-class Hydromedusæ or Medusæ craspedotæ, and presents the common characters of the order

Trachomedusæ (as distinguished from the Narcomedusæ) in having its genital sacs or gonads placed in the course of the radial canals. It agrees with all Tracholinæ (Trachomedusæ and Narcomedusæ) in having endodermal otocysts, and it further exhibits the solid tentacles with cartilaginous axis, the centripetal traveling of the tentacles, the tentacle rivets (Mantel-spangen), the thickened marginal ring to the disk (Nessel-ring) observed in many Tracholinæ.

Amongst Trachomedusæ, *Craspedacusta* finds its place in the Petasidæ, which are characterized as "Trachomedusæ with four radial canals, in the course of which the four gonads lie, with a long tubular stomach and no stomach-stalk."

Amongst Petasidæ it is remarkable for the great number of its tentacles, which are all solid; and for its very numerous otocysts. Further, it is remarkable among all Hydromedusæ (velate medusæ, that is, exclusive of *Charybdæa*) for the fact that centrifugal radiating canals pass from the otocysts into the velum, where they end caecally.

The genus may be characterized as follows:

MOUTh quadrifid, with four per-radial lobes.

STOMACH long, quadrangular, and tubular, projecting a good deal below the disk.

DISK, saucer-shaped, that is, flattened.

RADIATING CANALS 4, opening into the marginal canal.

GONADS 4, in the form of 4 oval sacs, depending into the cavity of the subumbrella from the four radiating canals.

MARGINAL or RING CANAL voluminous.

CENTRIPETAL CANALS (such as those of *Olindias*, *Geryonia*, etc.) absent.

TENTACLES solid; in three sets, which are placed in three superimposed horizons:—

1. A set nearest the aboral pole, of 4 large per-radial tentacles. These are the *primary* tentacles.
2. A second tier of (in large specimens) 28 medium-sized tentacles placed between these in four groups of seven. These are the *secondary* tentacles.
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TENTACLE-RIVETS (Mantel-spangen) connecting the roots of the tentacles with the marginal ring (Nessel-ring) are connected with all the tentacles of each of the three horizons.

OTOLITHS placed along the line of insertion of the velum—about eighty in number (fewer in small specimens). From sixteen to twenty are placed between successive per-radial tentacles arranged in groups of two or three between the successive secondary tentacles.

VELAR CENTRIFUGAL CANALS (which are really the elongated otocysts) are peculiar to this genus, passing from the otoliths (one inclosing each otolith) into the velum, and there ending blindly. They appear to correspond in character to the *centripetal* canals found in other Trachomedusæ in the disk.

OCELLI are absent.

[The presence of velar otocystic canals constitute the chief peculiarity of the genus *Craspedacusta*, and may necessitate the formation of a distinct family or sub-order for its reception. The minute structure of the otoliths and canal-like otocysts I am now engaged in investigating.]

The above characters are derived from the examination of *adult* male specimens, which were freely discharging ripe, actively motile spermatozoa.

The species may be known as *CRASPEDACUSTA SOWERBII*, nov. gen. et sp.—I name the species in honor of Mr. Sowerby, who discovered it, and to whose quick observation and courteous kindness zoologists are indebted for the knowledge of this interesting animal.

The sole character which I can give as specific over and above the generic characters summarized above is that of size. The diameter of the disk does not exceed one-third of an inch.

Locality.—The water-lily tank in the gardens of the Botanical Society, Regent's Park, London.

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EVOLUTION OF LOCOMOTIVES IN AMERICA.

The question of priority in the use of the locomotive on railroads in this country is one of perennial interest. The literature on the vexed subject comprises volumes. For the seven cities of Greece, which claim the honor of the birthplace of Homer, we have had almost as many States claiming the honorable distinction of first introducing the locomotive engine for service on the railroad. The idea of applying steam as the motive power on railroads had occurred to many of our engineers, stimulated as they were doubtless by the successful practice of England; and the introduction of the locomotive by Pennsylvania and South Carolina was almost synchronous; yet the former is fairly entitled to the distinction of priority.

Fortunately there is now living in San Francisco one of the veteran railroad men of the country, who is absolutely familiar with the interesting incidents of the early history of the railroad and the locomotive engine in this country.

The testimony adduced from these intelligent and trustworthy sources is absolutely conclusive; and it would seem that it ought to end the controversy about the claim to priority in the first practical use of the locomotive engine in this country.

1. The first locomotive engine placed and tried on any railroad in America was called the "Stourbridge Lion," and was imported from England for the Delaware and Hudson canal and railroad company. This engine arrived in New York May 17, 1829, and was set up in the yard of the West Point foundry machine shops and publicly exhibited for days to thousands of the first citizens of the country. It was brought from England by Horatio Allen, who made the first experimental trial of it at Honesdale, on the banks of Lackawaxen creek, Pa., August 8, 1829, when he "opened the throttle valve of the locomotive engine that turned the first driving wheel on an American railroad." This highly interesting statement was made by Mr. Allen in a speech delivered at Dunkirk on the occasion of the celebration of the New York and Erie railroad.

2. The first locomotive built in America for a purely experimental purpose was the "Tom Thumb," which was constructed by the now venerable Peter Cooper. This little machine was built for the purpose of testing the feasibility of a locomotive sustaining itself while running over curves, which was a mooted point among the engineers and scientists of that day. The engine weighed less than a ton, the cylinder was only three and a half inches in diameter, the boiler was about as "large as an ordinary kitchen boiler," and was vertical, with gun barrels for tubes. The first trial was made on the Baltimore and Ohio railroad, from the depot at Baltimore to Ellicott's mills, August 28, 1830.

3. The first locomotive built in America for actual service on a railroad was called the "Best Friend," and was constructed for the Charleston and Augusta railroad company. This pioneer locomotive was built at the West Point foundry machine shops in New York City, and the work of fitting it up fell to the lot of Mr. Matthew. Immediately after the engine was completed it was placed on the company's road, and the first experiment with a train was made November 2, 1830, N. W. Darrell acting as engineer.

Some few days previous to the above date, or about the 20th of October, in accordance with a notice given in the Charleston papers, a public trial was made without any cars attached. It was on this occasion that the first American built locomotive turned its wheels for the first time on a railroad track. At the trial on November 2d the wooden wheels of the machine, which were constructed after the English practice, sprung and got off the track; but they were replaced by cast iron wheels, and on December 14th and 15th the engine was again tried and ran at the rate of 16 to 21 miles an hour with five cars carrying about 50 passengers, and without the cars it attained a speed of 30 to 35 miles an hour. In the Charleston *Courier*, March 12, 1831, there is an account of a later trial of speed of the "Best Friend," on which occasion, the writer remarks: "Safety was assured by the introduction of a barrier car, on which cotton was piled up as a rampart between the locomotive and the passenger cars." The second locomotive for service built in

this country was called the "West Point," and was for the same road. It was also constructed at the West Point machine shops.

4. The first locomotive built in America for a northern road was called the "De Witt Clinton," and was the third American locomotive. It was for actual service on the Mohawk and Hudson railroad. This engine, like the others, was built at the West Point machine shops, and was also fitted up by Mr. Matthew; and when it was completed he took it to Albany, June 25, 1831, and made the first excursion with a train of cars over the road August 9, 1831. According to Mr. Matthew's statement, the "De Witt Clinton" weighed $3\frac{1}{2}$ tons, and hauled a train of 3 and 5 cars at the speed of 30 miles an hour. It is especially noteworthy that both the cab and the tender of the "De Witt Clinton" were covered to protect the engineer from the weather—a "happy thought" of honest David Matthew, for which all American engineers at least ought to hold him in kind remembrance. About the middle of August the English locomotive, "Robert Fulton," built by the younger Stephenson, arrived and was placed on the Mohawk and Hudson road for service in the middle of the following September.

These locomotives had been used and fairly tested both on the southern and northern railroads, and the necessity for a radical change in their construction had become evident. Very soon John B. Jervis devised the plan of putting the truck under the forward part of the engine to enable it to turn sharp corners easily and safely. The machine so constructed was called the "bogie" engine. The first of these engines ever built was for the Mohawk and Hudson road, and was called the "Experiment." It was put on the road and ran by Matthew, who says it was as "fleet as a greyhound. The "Experiment" had been built to burn anthracite coal solely; after a while it was rebuilt and adapted to the use of any kind of coal, and its name was changed to the "Brother Jonathan." Shortly after these changes had been made the English locomotive "Robert Fulton," belonging to the same company, was also rebuilt and furnished with the truck, and named the "John Bull." The "Brother Jonathan" was a remarkable machine for those pioneer days. Mr. Matthew says of it: "With this engine I have crossed the Mohawk and Hudson railroad from plane to plane, 14 miles, in 13 minutes, stopping once for water. I have tried her speed upon a level, straight line, and have run a mile in 45 seconds by the watch. She was the fastest and steadiest engine I have ever run or seen, and I worked her with the greatest ease." This is certainly wonderful speed, and may be, as Matthew earnestly maintains it is, the fastest time at least on the American railroad record.

In comparison to the splendid and efficient engine of today, our first locomotives, built after the English model mainly, were clumsy and crude machines. Since then our improvements have been manifold and extraordinary, and the American locomotive is now pronounced the most "perfect railroad tool in the world." Its exquisite symmetry and flexibility, and its extraordinary powers must fill the mind of a veteran like Matthews—who has watched its growth from its infancy in this country—with feelings of generous admiration and pride. The English and American railroads and locomotives are strikingly contrasted by a writer in *Harper's Magazine* for March, 1879. English roads are short, solid, straight and level, and laid with the best rails in the world; and their massive and powerful, and rigid-framed engines are thoroughly adapted to those perfect roads. On the contrary, the American road is generally of great length, and being necessarily cheap it "goes as you please." Over these eccentric roads the American locomotive adjusts itself to every change of level both across and along the line; it takes curves that would be impossible for the rigid English engine; and, finally, it runs over a crazy track, up hill and down, in perfect safety. It has been well said that all that the English engine can do on a perfect road the American engine will do; and much more than this, it will do work on any road, however rough, hilly, curved and cheap. The name of the first American locomotive seems to have been inspired, for it has in the largest sense proved our "Best Friend."—*Cal. Scientific Press*.

LITERARY INTELLIGENCE.

We have received *The Microscopist's Annual* for 1879, published quite recently and dated 1880. It contains useful tables, rules, formulæ and memoranda, a list of microscopical societies, with officers, etc.; Directory of prominent microscope makers, dealers and importers in America and Europe. We trust that microscopists will patronize this thoroughly practical little work, and as it is issued by the Industrial Publishing Company, of Dey street, New York, at the nominal price of twenty-five cents, its expense can hardly be a bar to its purchase. We understand future numbers will be considerably enlarged.

THE announcement is made of a new bi-monthly magazine called *The Educational Review*, which will be devoted to the science and philosophy of education, in all its departments of thought and discussion. It will be conducted by Mr. Thomas W. Bicknell, whose great experience in educational literature cannot fail to make it a success, and worthy of the great subject it takes in hand.

GENERAL NOTES.

THE French journal, *La Lumière Electrique*, is to receive the addition of a supplement (having separate pagination) in which will be given a *résumé* of recent discoveries and inventions. For the present, these supplements will be confined to the subject of electric lighting.

IN a new form of telephone-receiver brought before the French Academy by M. Ader, two plates are used, arranged in such a manner that the air can pass through a central hole in one to the other. The result is, much louder tones, the second plate acting as a sort of soundboard.

THE Corporation of Yale College have established a horological laboratory in connection with the Winchester Observatory, with the view of encouraging the manufacture of more refined apparatus for the measurement of time.

IT is reported that Mr. Swan, of Newcastle-on-Tyne, has succeeded in rendering his little electric lamp a success. He uses a carbon thread in a vacuum tube, which supplies a soft and steady light, well adapted for household purposes.

M. DU MONCEL has just published a third edition of his work, entitled "*Le Téléphone, le Microphone, et le Phonographe*," the two previous editions (containing 5,500 copies each) having been exhausted in fifteen months. The numerous recent developments of the telephone and microphone are described, and 48 new engravings are added. The phonograph seems to have made but little progress since its appearance; M. Du Moncel, however, specifies a few improvements of it.

THE House of Lords' Committee have passed the preamble of the Bill for the construction of a subway available for all kinds of traffic, vehicular and passenger, under the Mersey, so as to connect the towns of Liverpool and Birkenhead. The total length will be 1 mile 6 furlongs $6\frac{1}{2}$ chains, and the estimated capital required £500,000, the taking up of which is guaranteed by the Corporations of Liverpool and Birkenhead, the Mersey Dock Board, and the Great-Western Railway, each of whom are prepared to give security for one quarter the cost of construction. The engineers are Mr. John Fowler, of London, and Messrs. Law and Thomas, of Wrexham.

IN a recent note to the Paris Academy, Professor Marangoni gives the results he has arrived at in a study of the swimming-bladder of fishes. He states, first, that it is the organ which regulates the migration of fishes, those fishes that are without it not migrating from bottoms of little depth, where they find tepid water; while fishes which have a bladder are such as live in deep, cold water, and migrate to deposit their ova in warmer water near the surface. Next, fishes do not rise like the Cartesian diver (in the well-known experiment), and they have to counteract the influence of their swimming-bladders with their fins. If some small dead and living fishes be put in a vessel three-quarters full of water and the air be compressed or rarefied, one finds in the former case the dead fish descend, while the living ones rise, head in advance, to the surface. Rarefying has the opposite effect. Fishes have reason to fear the passive influences due to hydrostatic pressure; when fished from a great depth, their bladders are often found to be ruptured. Thirdly, the swimming-bladder produces in fishes twofold instability—one of level, the other of position. A fish, having once adapted its bladder to live at a certain depth, may, through the slightest variation of pressure, be either forced downwards or upwards, and thus they are in unstable equilibrium as to level. As to position, the bladder being in the ventral region, the centre of gravity is above the centre of pressure, so that fishes are always threatened with inversion; and, indeed, they take the inverted position when dead or dying. This double instability forces fishes to a continual gymnastic movement, and doubtless helps to render them strong and agile. The most agile of terrestrial animals are also those which have least stability.

A new process of extracting sugar from molasses has been proposed by M. Gayon. It is based on the destruction of the glucose of molasses by fermentation; the sugar remains unaltered, and is obtained by ulterior crystallization. The ferment employed is a pretty common mould, *Mucor circinelloides*. M. Van Tieghem found it in horedung, and was the first to describe it. The ferment cells must not be confounded with those of beer-yeast, or *saccharomyces*. They differ in form, and, unlike beer-yeast, this *mucor* is powerless to produce glucosic and alcoholic fermentations of cane-sugar, whereas it acts like all alcoholic yeast on glucose and similar compounds. If, then, the cells of *mucor* be sown in a nutritive solution of cane-sugar and glucose, the latter alone ferments, the sugar remaining unaltered, whereas with beer-yeast all ferments. This conclusion was confirmed indirectly by experiments made with a view to ascertain the constitution of the inactive glucose of molasses by saccharimetric observation. M. Gayon has succeeded in fermenting 200 or 300 c.c. of molasses solution, and he remarks that by combining the process with osmosis one might, no doubt, extract, in the dry crystalline state, all the sugar which the glucose and the salts retain in molasses. (The Editor of the *Journal de Pharmacie* observes that it is only exceptionally that glucose exists in molasses in sensible proportion, and it is the salts that prevent crystallisation of the sugar; nevertheless, M. Gayon's researches are of much interest scientifically.)

THE blood of most slaughter-houses is usually dealt with in a primitive manner in open air, and without previous disinfection. This is obviously opposed to hygienic and economic laws. M. Vautelet has lately brought forward a process of treating all organic detritus from slaughter-houses for agricultural purposes. He uses sulphate of alumina, sulphuric acid, and nitric acid in fixed proportions. By addition of sulphuric acid to sulphate of alumina a bisulphate is formed, which, less soluble than the sulphate, quickly causes a complete coagulation of the blood. The rôle of the nitric acid is coagulation of the albumin of the blood and formation of nitrate. The matters are thus disinfected, and their fertilising power fully preserved.

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LITERARY INTELLIGENCE.

We have received *The Microscopist's Annual* for 1879, published quite recently and dated 1880. It contains useful tables, rules, formulæ and memoranda, a list of microscopical societies, with officers, etc.; Directory of prominent microscope makers, dealers and importers in America and Europe. We trust that microscopists will patronize this thoroughly practical little work, and as it is issued by the Industrial Publishing Company, of Dey street, New York, at the nominal price of twenty-five cents, its expense can hardly be a bar to its purchase. We understand future numbers will be considerably enlarged.

THE announcement is made of a new bi-monthly magazine called *The Educational Review*, which will be devoted to the science and philosophy of education, in all its departments of thought and discussion. It will be conducted by Mr. Thomas W. Bicknell, whose great experience in educational literature cannot fail to make it a success, and worthy of the great subject it takes in hand.

GENERAL NOTES.

THE French journal, *La Lumière Electrique*, is to receive the addition of a supplement (having separate pagination) in which will be given a *résumé* of recent discoveries and inventions. For the present, these supplements will be confined to the subject of electric lighting.

IN a new form of telephone-receiver brought before the French Academy by M. Ader, two plates are used, arranged in such a manner that the air can pass through a central hole in one to the other. The result is, much louder tones, the second plate acting as a sort of soundboard.

THE Corporation of Yale College have established a horological laboratory in connection with the Winchester Observatory, with the view of encouraging the manufacture of more refined apparatus for the measurement of time.

IT is reported that Mr. Swan, of Newcastle-on-Tyne, has succeeded in rendering his little electric lamp a success. He uses a carbon thread in a vacuum tube, which supplies a soft and steady light, well adapted for household purposes.

M. DU MONCEL has just published a third edition of his work, entitled "*Le Téléphone, le Microphone, et le Phonographe*," the two previous editions (containing 5,500 copies each) having been exhausted in fifteen months. The numerous recent developments of the telephone and microphone are described, and 48 new engravings are added. The phonograph seems to have made but little progress since its appearance; M. Du Moncel, however, specifies a few improvements of it.

THE House of Lords' Committee have passed the preamble of the Bill for the construction of a subway available for all kinds of traffic, vehicular and passenger, under the Mersey, so as to connect the towns of Liverpool and Birkenhead. The total length will be 1 mile 6 furlongs $6\frac{1}{2}$ chains, and the estimated capital required £500,000, the taking up of which is guaranteed by the Corporations of Liverpool and Birkenhead, the Mersey Dock Board, and the Great-Western Railway, each of whom are prepared to give security for one quarter the cost of construction. The engineers are Mr. John Fowler, of London, and Messrs. Law and Thomas, of Wrexham.

IN a recent note to the Paris Academy, Professor Marangoni gives the results he has arrived at in a study of the swimming-bladder of fishes. He states, first, that it is the organ which regulates the migration of fishes, those fishes that are without it not migrating from bottoms of little depth, where they find tepid water; while fishes which have a bladder are such as live in deep, cold water, and migrate to deposit their ova in warmer water near the surface. Next, fishes do not rise like the Cartesian diver (in the well-known experiment), and they have to counteract the influence of their swimming-bladders with their fins. If some small dead and living fishes be put in a vessel three-quarters full of water and the air be compressed or rarefied, one finds in the former case the dead fish descend, while the living ones rise, head in advance, to the surface. Rarefying has the opposite effect. Fishes have reason to fear the passive influences due to hydrostatic pressure; when fished from a great depth, their bladders are often found to be ruptured. Thirdly, the swimming-bladder produces in fishes twofold instability—one of level, the other of position. A fish, having once adapted its bladder to live at a certain depth, may, through the slightest variation of pressure, be either forced downwards or upwards, and thus they are in unstable equilibrium as to level. As to position, the bladder being in the ventral region, the centre of gravity is above the centre of pressure, so that fishes are always threatened with inversion; and, indeed, they take the inverted position when dead or dying. This double instability forces fishes to a continual gymnastic movement, and doubtless helps to render them strong and agile. The most agile of terrestrial animals are also those which have least stability.

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